

STRATEGIC THEORY

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SYNOPSIS: We advocate a large increase in the funding for theoretical astrophysics by the NSF, NASA, and DOE. Some of this additional funding should be devoted to theoretical astrophysics that guides the conception and design of observational and experimental programs---“strategic theory”---an increasingly important, but largely unrecognized and unsupported part of our field. New programs to support strategic theory, or an augmentation of existing programs for this purpose, would provide great benefits to the theoretical astrophysics community and would enhance the scientific impact of the observatories, missions, and laboratories of the NSF, NASA, and DOE for just a tiny fraction of the cost of these facilities.

Much of theoretical astrophysics falls into two broad categories. One, sometimes called “pure theory”, involves studies of fundamental processes that underlie a wide variety of astrophysical phenomena (e.g., using the ideas and methods of radiative transfer, magnetohydrodynamics, stellar dynamics, etc. to uncover the structure of astrophysical systems). The other, sometimes called “phenomenology”, is tied more directly to the interpretation of data sets generated by ongoing observational or experimental programs. This distinction is reflected in the structure of the NASA grants program: the Astrophysical Theory Program is designed to support the former, while the Archival Research Programs of the Hubble, Chandra, and Spitzer missions invite proposals to do the latter.

Increasingly, however, theoretical astrophysics contributes to our discipline in another way, one that often goes unrecognized and unsupported: theory has aided significantly in the conception and design of new observational and experimental programs, including the associated observatories, missions, and laboratories. We call this activity “strategic theory”, and we advocate that it now be recognized and supported in a substantial and sustained manner.

Before we discuss the possible forms this recognition and support might take, we first review briefly some of the different ways strategic theory contributes to astronomy and astrophysics and illustrate these with a few concrete examples from the recent past.

For many observations and experiments, theoretical concepts define the very terms of discussion. Long before temperature fluctuations were seen in the microwave background radiation, theorists pointed out that such fluctuations must be present, the result of acoustic oscillations in the primordial plasma, and that they would carry information about the origin and fate of the universe. WMAP was built for the sole purpose of measuring these signals and succeeded spectacularly. LIGO and LISA have equally strong theoretical underpinnings; everything we know or believe about gravitational

waves comes from the theory of general relativity (and alternative theories), without which such observatories would never have been contemplated. Similarly, two of the approaches being studied for JDEM, baryon acoustic oscillations and weak gravitational lensing, are entirely theoretical inventions. Absent the ingenuity of theorists, neither of these methods would exist.

Another important way in which theorists contribute to observational and experimental programs is by posing specific questions to investigate. When theorists construct models of astrophysical phenomena, they often identify key predictions whose observational confirmation or refutation would lead to either greatly strengthened confidence in these ideas or their rejection. RXTE, for example, was initiated in part on the basis of theorists' suggestions that sensitive, high time-resolution X-ray monitoring could probe the strong-gravity regime around black holes and neutron stars, testing theories of accretion disk dynamics and nuclear equations of state. Another good example is JWST, which was designed largely to test theorists' predictions of when and how galaxies formed. Alternatively, some theoretical models are complete except for a small number of measurable parameters, making those measurements important goals for new programs, WMAP and JDEM being prime examples.

Yet another way in which theorists stimulate new observations and experiments is by calculating the signals that might be seen by instruments with new capabilities. By extrapolating from what is now known using physical principles, they can suggest what might be measured by an instrument designed to achieve a certain flux sensitivity, wavelength coverage, or angular, spectral or temporal resolution. Conversely, such efforts can also indicate, at least at the order of magnitude level, how much these capabilities would have to be improved to make even more exciting or novel measurements. For example, one of us is currently exploring the science that might be possible with the recently developed instruments that could, for the first time, make X-ray spectropolarimetry possible for dozens of objects.

Theoretical ideas can also be crucial to designing optimal strategies for data reduction and parameter estimation. Consider, for example, the essential role theorists have played in planning for LIGO and LISA, computing gravitational waveforms for black hole inspirals and mergers and making population estimates of likely sources. Theorists also develop advanced mathematical or numerical techniques to extract signals efficiently from large data sets. A new algorithm for angular correlation analysis reduced the computing time required to analyze WMAP data by a factor of 3×10^7 , transforming an effectively impossible task into one that was eminently feasible.

Strategic theory often falls in the cracks between the existing, better recognized and better supported categories of theoretical astrophysics. It usually has too much of an applied flavor to fit into the pure-theory programs; and it usually is too future-oriented to fit into the data-interpretation programs sponsored by operating observatories and missions. Indeed, the official language describing the NASA Astrophysical Theory Program discourages it:

“Theoretical proposals to the ATP program may not:

- propose to develop technologies or experimental concepts for future NASA missions
- primarily aim at studying new mission concepts in preparation for future”

As a result of policies like these, some of the strategic theory done hitherto has not been supported at all, while some has been supported indirectly by grants awarded mainly for other purposes. The little direct support that has been available has come mostly in the form of small grants awarded to members of project advisory committees or science teams, a level of funding not adequate for detailed or difficult calculations (which often require the assistance of graduate students or postdocs). The only exception to this pattern is that a number of proposals for support of LISA science have been funded through NASA/ATP. Moreover, this type of support usually becomes available only after a project has entered the design phase, long after its basic character has been determined.

Strategic theory would be more effective and valuable if it were supported in more substantial and sustained ways, and that is the thrust of our proposal. The scientific impact of observational and experimental programs depends crucially on whether they address fundamental or only incidental issues, and this is where the guidance from theorists can be very helpful. Strategic theory, for a tiny fraction of the cost of major observatories, missions, and laboratories, can greatly enhance the scientific returns from these facilities (in the same way that good architecture contributes disproportionately to the quality of a building). Funding for this important activity, however, is almost nonexistent, as we have already noted.

The need to encourage and support strategic theory seems clear to us. The best way to provide this encouragement and support is less obvious. In the following, we sketch out two options, but we realize that there may be other ways, equally good or even better, to achieve the same ends. It is also possible that the different situations of the different agencies may lead to different solutions for each (for example, NSF, unlike NASA, does not separate theory from observation in its grants program). This issue deserves further discussion and refinement by a larger group of interested people.

Irrespective of which option is adopted, a large part, perhaps half, of the funds for strategic theory should be devoted to “blue-sky” studies aimed at generating new observatory and mission concepts, exploring theoretical astrophysics issues raised by these concepts, and evaluating the likely scientific impact of the observatories and missions. In effect, these theoretical studies would act as a partial filter, helping to identify the most scientifically promising observatory and mission concepts. We are not advocating that theoretical appeal be the only criterion for observatory or mission approval---other important criteria include discovery potential, technical readiness, and cost---but we do believe it is important to understand as much as possible the likely theoretical implications of every major observatory or mission before it enters the design phase on the path toward possible construction, launch, and operation.

The balance of funds for strategic theory should be devoted to more focused studies on behalf of observatories and missions that are approaching or have already entered the design phase. Ideally, such studies would be carried out by the members of the science working group of the observatory or mission. We believe that each science working group should have several theorist members with relevant expertise and that they should receive enough support to address in some depth most of the theoretical issues that arise during the design phases of the observatory or mission. In practice, however, these ideals are rarely attained. Thus, we recommend that some support be made available to theorists outside the science working groups for studies relevant to observatories and missions in the design phase.

One way to increase support for strategic theory is to augment the budgets of the existing grants programs for theoretical astrophysics in the NSF, NASA, and DOE expressly for this purpose. This approach has the advantage that the proposals within different categories of theoretical astrophysics (pure theory vs strategic theory, or, in the case of NSF, against observational programs as well) could be judged directly against each other and the proportion of successful proposals of each type could then be determined by scientific merit alone. A possible disadvantage of this approach, however, is that, in the face of budget pressures, it may be harder to maintain an adequate overall level of support for theoretical astrophysics if each agency has only a single general-purpose theory program.

Another way to increase support for strategic theory is for each of the agencies to create a separate grants program for this purpose, distinct from their pure-theory and data-interpretation programs. This may be the best way to give strategic theory the encouragement and support it deserves, and to recognize its distinct and increasingly important role in our field. This option would also make it easier to allocate appropriate levels of funding for theoretical studies pertaining to the conception and design phases of observatories and missions (discussed in the preceding two paragraphs).

However, if the NSF, NASA, and DOE create separate strategic theory programs, this must *not* be done at the expense of their existing theory programs. It is likely that the strategic theory programs would spin off some “pure theory”. But such programs are not primarily aimed at theory for its own sake, and must not be regarded as substitutes for healthy, well-funded theory programs that have no ties or restrictions to observatories or missions. Therefore, we strongly advocate that, if the NSF, NASA, and DOE create separate strategic theory programs, as outlined here, that they also maintain or enhance the support for unrestricted theory within their existing programs.

Whichever option is chosen to support strategic theory---augmenting existing programs or creating new ones---theorists will be better equipped to help with the conception and design of new observatories and missions. Our proposal builds on the important contributions theorists have already made in this area, strengthens these efforts, and prevents them from becoming haphazard. The outcome, for relatively little cost to the agencies, but a large augmentation of funding for the theory community, will be higher quality observational and experimental programs.

